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Trade and Culture Process in European Prehistory¹

by Colin Renfrew

TRADE IS ONE OF THE ACTIVITIES of prehistoric man which has received much less attention than it deserves. Its particular and sometimes crucial importance lies in a dual status: as the indicator for us today that intercultural contact was taking place, and as a prime motive, among prehistoric groups, for such contact. In this paper an attempt is made to indicate the unique opportunities which a study of trade holds for the understanding of culture change. The examples and discussion are limited in scope to European and Near Eastern prehistory, but the underlying principles are general ones and should bear upon the prehistory of any region.

The first point, trade as an indicator of contact, implies a proof that trade (or at least the transport of goods from one region to another) was taking place. In the past, identification of unique products has sometimes been made on the basis of simple inspection. Lapis lazuli, Polish banded flint, Grand Pressigny flint, and Olonetz slate (Clark 1952: 246–47) are examples of raw materials which can very probably be assigned to a limited source area on the grounds of visual inspection alone. To these well-known cases can be added, for example, African ostrich egg shell and ivory in early Iberia (Renfrew 1967a). Amber (sources in northern Europe, Iberia, southern France, Sicily, Rumania, and points east) and *Spondylus* shell (available in the Adriatic

and perhaps the Black Sea as well as the Aegean), however, are commodities whose significance may have been misinterpreted in the past (de Navarro 1925; Clark 1952: 242, 263). A proper interpretation depends upon the scientific characterisation of the material.

Trade as a motive for contact, and the entire question of intercultural contact in the New World as well as the Old, is wrapped in confusion. There has been little serious discussion of prehistoric trade mechanisms and virtually no attempt to set up the facts on a quantitative basis. Finally, there has been little attention to the role of trade, an important element in economic growth, as a causative factor in cultural change within given regions.

The present paper, in seeking to open up discussion on the role of trade in culture contact and culture development, has three central points to make:

1. Much of our thinking on the subject of cultural change in the prehistory of Europe and beyond is outdated and inadequate. Too much emphasis has been placed on supposed invasions and diffusion from “higher centres of civilisation” and insufficient on the economic and social development of what may have been, for many purposes, essentially independent systems. The evidence for supposed outside influences must be re-examined and an attempt made to understand the factors which work towards change within society.

2. If the explanation for an observed change in the prehistoric record is generally to be sought within the community or culture rather than outside it, this implies an understanding, preferably in quantitative terms, of various factors or systems, particularly economic systems, which operate within the cultures. Trade is only one such factor, but one of the easiest to evaluate.

It is here suggested that the economic importance of trade in European prehistory has been misinterpreted in several ways. There has been a tendency to overdramatise the scope and range of trade in early periods; to see traders and prospectors from Sumer influencing the development of prehistoric Europe, and Mycenaean

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This paper, submitted for publication 16 x 67, was sent for CA☆ treatment to 50 Associates, of whom the following responded with comments: Elizabeth Chesley Baity, D. K. Bhattacharya, Lewis R. Binford, B. Brentjes, George Dalton, Carl-Axel Moberg, Philip E. L. Smith, Julian H. Steward, Saul S. Weinberg, and Roscoe Wilmeth. Their comments are printed after the text and are followed by a reply from the author.

¹ The first draft of this paper was read at the Research Seminar in Anthropology and Related Subjects at the Institute of Archaeology, University of London, on November 7, 1967. I am grateful to Lewis Binford, George Dalton, and Frank Hole for helpful comments on the original text.

princes sending their emissaries to the barbarian North. These romantic ideas of early tele-commerce, where a civilised region colonises, at least economically, an advanced one, have tended to obscure the real and very important role which trade seems to have played in developing the economic life within particular cultural areas, progressing and growing quite independently of outside influence. Also obscured has been the fact that trade, as an agent in communication, and hence in establishing areas of cultural and material exchange, had a cultural significance which long preceded its economic one.

3. Trade cannot be assumed; it has to be proved. For instance, the amber trade between the Baltic and the Aegean in the Mycenaean period, the cornerstone of many a theory and of chronologies, may yet prove to be a myth. Again, the popular notion of a metal trade between Iberia and the Aegean in the 3rd millennium B.C., supposedly a cause of Iberian development and even of the whole phenomenon of the European megaliths, could well be without foundation. A considerable range of scientific techniques is now available for the characterisation of materials and the determination of their sources, so that the foreign nature of a traded commodity need no longer be in dispute. If such determinations can be linked with an estimate of the quantities involved (and factors of preservation make an obvious difficulty), we shall be in possession of prehistoric trade statistics. Already the subsistence of any prehistoric society can be known to us, with a little trouble, on a quantitative basis. With a quantitative knowledge of its trade also, we shall come much closer to an understanding of the factors which may work towards stability or change in the society.

Before proceeding further, however, it is important to explain the meaning which is here attached to the term "trade." Although for early historic times the existence of full-time professional traders can sometimes be documented, it can certainly not be assumed for prehistoric societies. Professional trade and commerce was probably absent from most prehistoric communities. "Trade" is therefore to be understood in its widest sense: the reciprocal traffic, exchange, or movement of materials or goods through peaceful human agency. The reciprocity cannot always be demonstrated, but it can often be assumed, when booty, tribute, or tax seem unlikely. At least it makes clear that the goods must change hands; this I take to be the crucial defining feature. When goods are given, unless they are given under duress or as "unsolicited gifts," something is received in return. This need not imply a commercial transaction, but simply a mutually satisfactory exchange—a process which can be repeated several times for the same commodities. The full-time trader, a non-producing middleman deriving his livelihood or sustenance entirely from these exchanges, is often the product of more recent and more advanced societies. For this reason ethnographic parallels have to be used with extreme care, and comparisons between prehistoric colonisations and trade and those of European commerce in the world during or since the Middle Ages are of doubtful validity. Thus when the terms "commodity," "profit," or "surplus" are used below, they are not intended to imply commercial transactions or economic structures like those of our own time. Evidently

different levels and modes of economic organization are served by different kinds of trade. To define the word too rigorously or narrowly is to exclude the wide variety of transactions and exchanges which are the subject of the inquiry.

In the first section which follows, the vague and conflicting models in current use in the study of prehistory, both in Europe and beyond, for the explication of culture change are briefly cited. The second section surveys in outline some of the methods of characterisation now available for the scientific documentation of cultural contact. The third section mentions some of the scattered and incomplete attempts which have been made in Europe at the quantitative assessment of archaeological materials from an economic standpoint. Finally, one test case is considered where, contrary to current standard opinion, trade within a region, rather than influence from outside, seems to have been a major factor in the significant change of the way of life from "pure Neolithic" to "urban."

MODELS FOR CULTURAL CHANGE

THE INVASIONIST MODEL

The invasionist/migrationist model, when applied to a particular case, will suggest that an apparently rapid and marked change in culture is explicable by the arrival of a new group of people (Daniel 1962: 82). To give modern instances of the application of this model—which is obviously an entirely valid one in some instances—it is not necessary to take extreme cases. Sir Grafton Elliot Smith's notion of wandering Egyptians (Elliot Smith 1929) responsible for the origin and development of all civilisations everywhere is today a curiosity hardly worth attacking. The idea that the transition in Britain to a Neolithic way of life (Piggott 1954) is an instance of invasion-change, is today widely accepted, although later British developments are subject to considerable controversy (Clark 1966: 172). Again, the earliest Neolithic settlements in southeastern Europe are generally regarded as belonging to an intrusive population, although Rodden (1965: 86) has discussed other possibilities.

Most such instances, however, are less acceptable. It has been held that the Aegean Early Bronze Age was introduced from the southeast, financed by Mesopotamian capital (Childe 1957: 19–21), and several scholars suggest an invasion at this time (Haley and Blegen 1928; Weinberg 1965b: 302–4). The situation is here complicated by linguistic arguments, but the evidence for outside influence is, in fact, very tenuous (Renfrew 1964); often the invasion is assumed rather than demonstrated. The Iberian Chalcolithic, and sometimes all of the European megaliths in addition, have been considered the product of colonisation, and similar views are often propounded for the southeastern European Chalcolithic also (Blance 1961, Popovitch 1965). A diffusionist theory is sometimes preferred in these cases, but rarely is the mechanism of the supposed transmission considered.

Very rarely is any systematic attempt made to consider how an invasion would emerge in the archaeological record, or how further relevant evidence could be

elicited. A new pottery style, e.g., "Minyan" ware in Middle Helladic Greece, is often accepted as enough; or a widespread destruction, such as befell the Late Minoan IB settlements of Minoan Crete. The ease with which these views can be abandoned for another dramatic and perhaps equally suspect theory (for example, the Santorini catastrophe as the cause of the Minoan destructions and decline) is symptomatic of the uncertain state of our thinking.

THE DIFFUSIONIST MODEL

The diffusionist model holds that changes observed in a society are brought about by strong influences from a "higher culture," without the significant transfer of population. (The term "diffusion" in its broader sense sometimes includes migration also.) Gordon Childe was the arch-diffusionist in recent European archaeological thinking; of his book *The Dawn of European Civilisation*, he wrote (Childe 1956a: 70), "The sole unifying theme was the irradiation of European barbarism by Oriental civilisation . . ." Of course, he was in a different class altogether from the now ludicrous Kossinna or Elliot Smith. To him, the "secondary and tertiary civilisations" of Europe were the product of "economic imperialism" on the part of the "primary civilisations" of the Near East (Childe 1956b: 176-77). Axiomatic to such a view is "the useful heuristic hypothesis that each invention has been made but once," and with its aid, Childe was able to fit the prehistory of Europe into a diffusionist mould. So pervasive is its influence that few prehistorians today can contemplate equably the possibility that metallurgy may have originated independently in Iberia and the Balkans as well as in the Near East.

The problem of distinguishing the products of diffusion from those of independent invention is of course a very old one (Galton 1889), and it has been reviewed several times recently (e.g., Rowe 1966). In much of European prehistory, and especially in the Bronze Age, the diffusionist model implicitly underlies and supports the very framework of the cultural succession itself, the supposedly factual data on which further conclusions are based.

THE EVOLUTIONIST MODEL

The evolutionist model seeks to illuminate a development in one place or period by comparing it with a supposedly similar development elsewhere. It is felt that the process of viewing the different developments jointly as products of the working (as yet unexplained in detail) of multilinear evolution has some explanatory power. Of course the old prescriptive Three Ages, like Morgan's seven-point scale of Savagery, Barbarism, and Civilisation, have been widely abandoned, and few would try to make all civilisations jump through so narrow an evolutionist hoop. Nonetheless some anthropologists seek to retain the notion of some universal cultural causality and law. Steward (1949) drew up a system of developmental regularities of early civilisations (Formative, Regional Florescence, Initial Conquest, Dark Ages, Cyclical Conquests, etc.), while a similar system (Lithic, Archaic, Formative, Classic, Postclassic) is widely used by American archaeologists today (Willey and Phillips

1958). Steward himself precluded criticism of this kind of theorising with a pre-emptive strike: "theories are not destroyed by facts—they are replaced by new theories." It is relevant to ask just what explanative power the multilinear evolution hypothesis in fact has to command. When the development of two remote and presumably independent societies shows features which are in some ways analogous, the similarity is explained as the product of multilinear evolution; but often this appeal to the superorganic implies no more than the "general truth" that independent societies often develop through apparently similar sequential stages. The "explanation" in such a case consists of reiteration of the data which it is supposed to explain: it is tautologous.

These grand, prescriptive, and sometimes inaccurate generalisations of multilinear evolution are not often based on any very detailed understanding of the factors underlying changes in the more complex societies, although Steward was himself a pioneer in such studies among hunting bands. We are not ready for them; the first need is to know more of the way specific prehistoric societies developed and changed.

THE CULTURE-PROCESS MODEL

A recent development in American archaeology has been the insistence that culture or cultures cannot usefully be studied as units, as in the above three cases, but must be regarded rather as aggregates of systems, each of which varies independently. Such systems must be studied in isolation in order to contribute to an understanding of the whole. Binford (1965: 203) has criticised, with justice, an outlook, largely shared by European prehistorians, in which cultures are indivisible units and "culture influence" is a term which can meaningfully be applied to the culture, and changes in culture, as a whole. He has stigmatised this as the "aquatic view of culture," where archaeologists think and speak rather vaguely in terms of a "culture stream" and, as well as providing some stimulating and controversial case studies, he has urged them to isolate and study particular systems.

Because the study of particular systems corresponds so well to the way archaeologists, with their research focus on ecosystems and human adaptations to them, are in fact working today, it promises to be particularly useful. It is no longer possible today to view a culture change, such as for instance the origin of urban settlement in the Early Bronze 2 Aegean, simply as the adoption of a new set of artefacts, with a different pottery assemblage, brought in from some ill-defined centre of migration. The interior workings of the society, particularly its economic subsystems, have to be understood. The material cultural assemblage is not likely to change significantly without a change in these subsystems, whether in agriculture, stock-rearing, fishing, or other subsistence occupations, in metallurgy, trade, or social organisation.

Systems of trade or exchange form only one component in the aggregate of systems which constitute a culture, but they play an important, and particularly complex, role. Among its multiple functions, trade works (a) as a source of wealth to the trader and to the community, since efficient or favourable exchange can encourage the

production of, or produce, a surplus; (b) towards specialised and hence more efficient production; (c) to promote contact between communities and the interchange of ideas through personal contact; and (d) to create new demand and ultimately the production of new commodities (e.g., silk in the Classical world or metal in prehistoric Europe). The last two factors always operate towards change rather than conservatively; thus trade facilities change through the reception of outside ideas.

These functions have deliberately been phrased in terms applicable equally to modern commerce. They certainly do not imply, however, the existence of modern commerce. None of them is restricted to commercial transactions, and all can be seen at work, for instance, in the Aegean of the 3rd millennium B.C., the Chalcolithic of Iberia, and probably the Wessex culture of the British Early Bronze Age. Not all of them need operate in any one case—perhaps only the third applies to the obsidian trade in the Near East in the 8th millennium B.C. Nevertheless, when culture change is the subject of inquiry, trade should usually be considered in the first instance.

THE INVESTIGATION OF TRADING SYSTEMS

Trade is of two kinds; of material commodities whose natural distribution is limited (e.g., salt, tin, amber, wine, opium, etc.) and of products which, through superior know-how or other economic factors, are most efficiently produced in a limited area (e.g., Hittite iron, slaves, Swiss cuckoo-clocks). In prehistoric times the first kind was more common, for despite Childe's insistence on the economic imperialism of the skilled Sumerians, there is only sporadic evidence for a trade in commodities where manufacturing skill was the primary factor until the export of Greek vases to Etruria and Etruscan jugs to the Celts. It follows, therefore, that prehistoric trade objects are to be recognised by their material more often than by their style.

A good example is the Egyptian stone bowls of Early Minoan Crete, whose stylistic evaluation has led to disagreement, while petrological examination is often conclusive. Again, the frequent use of ivory in Crete at that time documents contact, probably with Syria, while the design alone of the seals and other ivory objects would be open to stylistic argument.

The considerable battery of scientific methods available for the characterisation study of materials now includes examination by the following techniques:² simple inspection (materials of unique appearance, e.g., lapis lazuli, ostrich egg); petrological examination of thin section (e.g., stone tools, pottery, etc.); optical spectroscopy (e.g., pottery, metals, obsidian, faience, etc.); X-ray fluorescence (e.g., metals, obsidian, glass, jade, etc.); neutron activation (e.g., metals, pottery, obsidian, etc.); β -ray back-scatter (e.g., glass, glaze); infra-red absorption (e.g., amber); cathode-luminescence (e.g., marble); X-ray diffraction (e.g., jade, emery). Of importance too are various methods of dating the formation

of materials, which can aid in their characterisation, e.g., determination of the isotopic composition of lead by mass spectrometer, or fission-track dating of obsidian.

The possibilities and difficulties encountered with several of these commodities are of particular interest:

Amber. The well-known pattern of an amber trade from the Baltic to the Mediterranean may yet be disrupted by a more careful consideration of the problem, setting aside the old and inaccurate succinic acid determinations and taking into account other sources throughout Europe (Beck *et al.* 1965). Conclusions of considerable importance for the chronology and interpretation of the Central European Bronze Age are based upon this amber trade, and its validation (or modification) is an urgent need in European prehistory.

Jade. The strange mystery of the source of the Breton/British axes, evidently traded in considerable quantity, remains unsolved, despite very detailed examination by W. Campbell Smith (1965). The investigation as so far conducted, however, is clearly along lines which will lead to an eventual solution: the absence of local sources is puzzling, but one feels that they must one day be discovered, probably in Brittany. Again, the results are of great importance for prehistoric trade, and this is a good example of an investigation, not yet complete, which promises exciting conclusions.

Copper. Several attempts have been made, by trace-element analysis, to pinpoint the sources of the copper used in the European Early Bronze Age to produce specific objects. The literature is now a vast one, and many thousands of analyses have been conducted (e.g., Junghans, Schroder, and Sangmeister 1960), but there have been several valid criticisms of the logic and statistical validity of the interpretative methods used (Butler and Van der Waals 1964). The value of the analyses for the understanding of early technology is undoubted, and within a limited region the technique can provide very valuable information (cf. Pittoni 1959). It seems, however, that variation in the trace-element composition of the ores within a given region and uncertainties and variation in the smelting methods employed make the attribution to a single source exceedingly difficult.

Obsidian. Recent work on obsidian has allowed the establishing of trade patterns in the Near East back to the 8th millennium B.C., with more isolated finds of earlier date (Renfrew, Dixon, and Cann 1966). These patterns of contact, from the earliest Neolithic period, are significant for the development and dissemination of the Neolithic way of life in the three "nuclear" regions of the Near East. The method is applicable in America, Australasia, and Africa, as much as in Europe and the Near East.

Pottery. Very impressive work has been carried out, both by petrological examination of thin sections and by trace-element analysis, in tracing imported potsherds back to their region of manufacture (Catling, Richards, and Blin-Stoyle 1963; Bennyhoff and Heizer 1965). This seems one of the most promising points of research development for the study of trade.

Successful documentation of a trading pattern in commodities such as these, even if established on a quantitative basis, does not, of course, adequately describe the trading system. All trade is a two-way process.

² Many of these methods are described in the periodical *Archaeometry*; see also Aitken (1961: 156–75) and Lucas (1962).

TABLE 1
CALCULATED MEAT WEIGHTS OF VARIOUS SPECIES REPRESENTED AT THE
NEOLITHIC SITE OF SALIAGOS IN THE CYCLADES

	CLIFF 17, Pt A			SQUARE N 3		
	Min. no. ^a of animals	Meat wt. ^b kg.	Meat wt. %	Min. no. of animals	Meat wt. kg.	Meat wt. %
Sheep/goat	17	490	6.6	22	640	19.1
Bovine	1	210	2.8	6	1,260	38.2
Pig	4	180	2.5	10	450	13.6
Tunny	48	6,500	87.9	7	950	28.5
Limpet (<i>Patella</i>)	1,900	6.5	0.01	1,970	6.9	0.03
Top shell (<i>Monodonta</i>)	340	0.4		650	0.7	
<i>Murex</i>	140	0.7		216	1.8	
Cockle (<i>Cerastoderma</i>)	45	0.1		53	0.1	

^a Minimum numbers calculated for livestock on the number of humeri found, for tunny on the number of vertebrae, 39 vertebrae per fish. Cephalopod remains were considered insufficient to allow of their incorporation in the table.

^b Meat weight for livestock is taken as half modern liveweight; meat weight for tunny is taken as 135 kg., a rough average for fish of length 0.6 to 1.8 m.

Unfortunately the reciprocal trade, which may have a different pattern of its own, is often less clearly documented in the archaeological record. In most cases, even when a distribution pattern of objects travelling in the other direction can be established, the actual equation of the two commodities, and a fortiori any notion of their relative value, is hypothetical. At present we are forced to rely on recent anthropological analogies (Herskovits 1952: 155-56; Clark 1965) or to imagine a simpler form of the systems which are documented for the early empires (Polanyi, Arensberg, and Pearson 1965; Oppenheim 1954; Mallowsan 1965).

QUANTITATIVE EVALUATION OF ECONOMIC DATA

The quantitative evaluation of archaeological materials is a relatively new development. Most of the work hitherto undertaken (e.g., that of Robinson and Brainerd, Tugby, Doran and Hodson, Sackett, Clarke, Dempsey and Baumhoff, Kuzura, Mead and Dixon, and Hole and Shaw)³ has been devoted to typological and taxonomic ends: the ordering of artefacts for chronological or taxonomic purposes. A separate and fascinating development is the classification of artefacts to reach conclusions bearing on social organization (e.g., the work of Deetz [1965] and of Hill [1966]). Rather similar in procedure is the recent interesting attempt to distinguish functional (and thus economic) groups in Palaeolithic assemblages (Binford and Binford 1966).

All these take as their starting point certain taxonomic features of the artefactual material. The quantitative analysis of non-artefactual material, as undertaken

notably by Heizer⁴ and by Meighan (1959) in the U.S.A., especially in conjunction with the ecological outlook well exemplified by Hole and Flannery (1967),⁵ offers a more direct insight into the economic factors at work. In Britain, E. S. Higgs has been influential in this direction (Higgs, Vita-Finzi, Harris, and Fagg 1967; cf. Higham 1967).

As an example of which I have personal knowledge, I would like to report a quantitative study of the animal remains from two excavation areas of the Neolithic site of Saliagos in the Cyclades (Evans and Renfrew 1968). Here the calculation of the meat weights (see Table 1)⁶ transformed the understanding of the site (cf. Shawcross 1967). The relatively small size of the tunny fish vertebrae, which contributed only a very small fraction of the osteological material recovered, entirely masked the huge proportion of the meat weight which they represent. With this unexpected result, the marine location of the site, and of others of the same culture, takes on a new significance. The appearance of the tanged point of obsidian as the principal type of the lithic assemblage at these sites can also be reinterpreted. Seen as an arrowhead, the dominance of this point was puzzling, since virtually no wild animals were represented in the faunal remains; but interpreted as the prong of a fishing spear or leister, it suggests the great importance of the seasonal tunny slaughter which, as elsewhere in the Mediterranean today, must have been the major economic event of the year during the life of the settlement.

So far, the economic approach in prehistory has very successfully focused attention upon the *function*, rather

⁴ For bibliography, see Heizer (1960) and Cook and Heizer (1965).

⁵ For the application of the concept of microenvironment to areal research in prehistory, see Coe and Flannery (1967).

⁶ Compiled using species identifications by I. M. Clegg, I. A. Kinnes, and E. S. Higgs (mammalian fauna), J. M. Renfrew, P. H. Greenwood, and P. J. Whitehead (fish), and N. J. Shackleton (molluscs).

³ See bibliography by Doran (1967); also Cowgill (1967), Soudský (1967), and Hole and Shaw (1967).

TABLE 2
THE PROPORTION OF GREY OBSIDIAN (GROUP 1g) AT EARLY NEOLITHIC SITES
IN THE ZAGROS AREA^a

	DEH LURAN AREA			Sarab	Jarmo
	Bus Mordeh	Ali Kosh	Mohammad Jaffar		
Total in sample	196	578	367	498	683
No. of Group 1g pieces	0	72	107	82	251
% Group 1g	0	12.4	29.4	16.8	35.7

^a Approximate dates for the sites are as follows: Bus Mordeh, 7000 B.C., Ali Kosh, 6500 B.C., Mohammed Jaffar, 6000 B.C., Sarab and Jarmo, 6500 B.C.

TABLE 3
VARIATION IN QUANTITIES OF OBSIDIAN PRESENT AT EARLY NEOLITHIC SITES IN THE ZAGROS AREA

	DEH LURAN AREA			Sarab	Jarmo
	Bus Mordeh	Ali Kosh	Mohammad Jaffar		
Vol. excavated (cu.m.)	70	144	165	50	2,700
Total chipped stone	19,574	23,231	23,934	40,864	93,239
Chipped stone/cu.m.	279	162	144	817	34 ^a
Obsidian/cu.m.	2.8	4.0	2.2	14.5	14.3
% obsidian/total stone	1.0	2.5	1.6	1.8	42.0
Mean weight of obsidian fragments (gm.)	0.30	0.41	0.39	0.53	0.2

^a The small amount of chipped stone per cubic meter at Jarmo is probably explained by the large volume of building debris, from *tauf* walls, at this site.

than just the form, of artefacts of organic as well as inorganic materials (Clark 1952). There have been very few quantitative, rather than merely qualitative, considerations, however, and those which have been made deal, like the above, with food residues. The enormous impact upon archaeological research which one can foresee for the quantitative understanding of archaeological residues (from coprolites to entire mounds) has not yet been felt.

The quantitative study of imported materials will one day be a basic approach in the study of trade. At present, however, there is pitifully little quantitative work to set beside the growing body of information bearing on food production in subsistence communities. Snodgrass recently evaluated, by means of an elegant table, the date of the real impact of iron metallurgy in northern Greece (Snodgrass 1965: 231). Stone and Thomas (1956), working with faience beads, and Hooker (1967), with Mycenaean pottery in Egypt, have usefully considered the *quantities* of imported material rather than simply its presence, but these studies, while adding significantly to our knowledge, are exceedingly simple compilations. They are only the pathfinders for what should be a whole new approach to the problem of economic development, trade, and cultural contact.

Obsidian has proved a very convenient material for the quantitative study of trade. In the first place, its point of origin can usually now be determined. Secondly, on most prehistoric sites, chipped stone is very abundant,

so that even when obsidian is, relatively speaking, rare, enough pieces may yet be found to make statistical treatment possible. The data on the Early Neolithic obsidian finds in the Zagros area of the Near East (see Tables 2-4) make a very suitable subject for discussion (Hole, Flannery, and Neely 1968). Characterisation study establishes that no obsidian was reaching the Zagros area from central Anatolia at this time; the obsidian found is of groups 4c and 1g, from two different sources in eastern Anatolia. The following features are noteworthy: (1) The proportion of obsidian falls off to the south, away from the sources. (2) Little or no group 1g obsidian reached Deh Luran in the earliest, Bus Mordeh, phase. (3) In the Deh Luran area, the chipped stone industry possibly diminishes at the end of the Bus Mordeh phase, although the obsidian component increases both relatively and absolutely. (4) The increase in proportion at the end of the Bus Mordeh phase correlates with an increase in the mean weight of fragments. This may be due to a functional or culturally determined change, rather than to a simple increase in abundance, since the mean weight does not decrease greatly when obsidian becomes rarer again in the succeeding, Mohammad Jaffar, phase. (5) The very approximate calculations allow an estimate of the total weight of obsidian contained at the sites. At first sight the totals do not appear large; but it must be taken into consideration that there were no pack animals at the time and transport was by river or by human agency alone.

TABLE 4
TOTAL CALCULATED WEIGHTS OF OBSIDIAN AT EARLY NEOLITHIC SITES IN THE ZAGROS AREA

	DEH LURAN AREA			Sarab	Jarmo
	Bus Mordeh	Ali Kosh	Mohammad Jaffar		
No. of pieces in sample	196	578	367	723	39,211
Sample as fraction of total site	1/338	1/338	1/135	1/50	1/25
Size of site (cu.m.)	24,000	49,000	22,000	2,500	68,000
Total calculated weight of obsidian (kg.)	20	80	19	18	196

This example of a quantitative consideration of trade at an early date is, of course, a very simple one, but it leads directly to a more sophisticated analysis on a larger geographical scale. The fall-off in the proportion of obsidian in the total chipped stone industry, as the distance of the sites from the sources increases, has already been mentioned. Plotted on a graph this seemed so marked as to warrant replotting with the axis for “% Obsidian” (in the total lithic assemblage) on a logarithmic scale (Fig. 1). Sites indicated by squares lie along the Zagros range of Iran and Iraq, and were obtaining their obsidian from sources in eastern Anatolia, as trace element analysis has shown (Renfrew, Dixon, and Cann 1966). Those indicated by triangles lie in central Anatolia and the Levant, and obtained their obsidian from the central Anatolian sources. The figures are discussed in detail in a more recent paper (Renfrew, Dixon, and Cann 1968).

For present purposes, the details of this trade in the 7th and 6th millennia B.C. are less significant than the pattern established. In each case, within a distance of about 300 km. from the source—delimiting a region which I have called the supply zone—sites were well supplied with obsidian, which formed more than 80% of their lithic assemblage. Outside the supply zone the percentage falls off steeply. If all the points fell in a straight line, the fall-off would be exponential with distance from the edge of the supply zone. As the points do not form a perfect line, in both cases it may be more appropriate to see the fall-off as proportional to the fourth or fifth power of the distance from the supply zone. In any case, the pattern is very striking. This fall-off in what we have termed the contact zone may fruitfully be compared with similar sharp declines with distance recorded by economic geographers. The lapse rate in the number of truck trips around Chicago or of world ocean-going freight, when plotted on log/log or log/linear axes, gives a strikingly similar pattern (Haggett 1965: 34).

It is clear that the application of the techniques of locational analysis to archaeological materials promises a deeper insight into the working of prehistoric economic systems. So long as the data are quantitative, their limited accuracy may not be a bar to the emergence of valid order-of-magnitude patterns. In this instance it is tempting to correlate the supply zones with the locus of the two archaeological cultures within which the natural sources lie—a notion corroborated by the independent study of tool typology and distribution (Bialor 1962: 43).

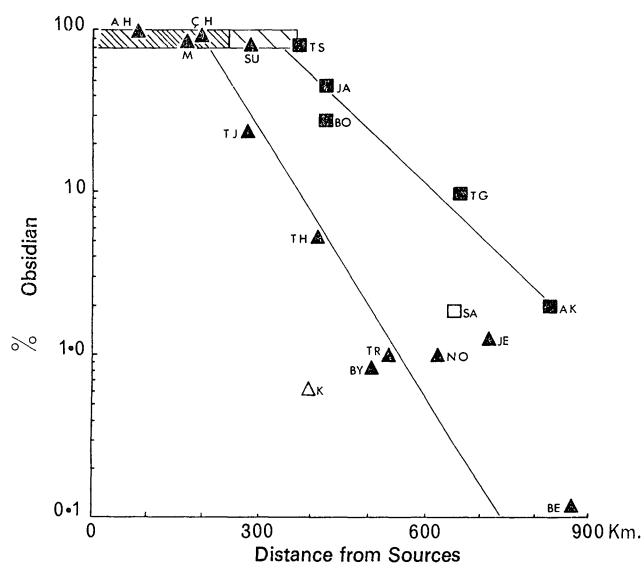


FIG. 1. Percentage of obsidian in the total chipped stone industry, plotted on a logarithmic scale against distance from source, for Early Neolithic sites in the Near East, ca. 6500 to ca. 5000 B.C. Triangles indicate sites in central Anatolia and the Levant (supplied by central Anatolian sources), squares those in the Zagros area of Iraq and western Iran (supplied by eastern Anatolian sources). Shaded areas indicate the supply zones; the straight lines show the approximately exponential fall-off in the contact zones.

The decline in the contact zone can be simulated by at least one model for the exchange mechanism (Renfrew, Dixon, and Cann 1968) and certainly excludes some other mechanisms which might otherwise have been thought plausible.

So far, the more sophisticated statistical and locational analysis techniques have not, other than for seriation purposes, been much applied on any large areal scale, and attention has focused on individual sites and assemblages. The study of an abundant commodity, such as obsidian, which is widely (although not uniformly) distributed through individual sites, and among sites in a given region, is clearly an easy task compared with some. For finds of a more sporadic nature, some sort of sampling procedure might be applied to combat the irregularities in archaeological preservation.⁷

These figures are prehistoric trade statistics; we know where the material was coming from, in what quantity,

⁷ See the discussion by Binford (1964) and the sustained application to a single site by Binford, Binford, Whallon, and Hardin (1966).

and when. In the 7th millennium B.C., the obsidian trade was probably not of great economic significance; but if a similar approach could be applied to different commodities, and later periods, when the volume of trade was greater, our understanding of prehistoric economic processes would take on an entirely new dimension.

TRADE AND URBANISATION

Hitherto, as probably for some time to come, the detailed quantitative study of a complete regional economic or trading system has not been possible. A qualitative approach with the same objectives seems useful as an interim measure. The example presented here may help in the understanding of urbanisation in general. It has often been argued that urbanisation can only take place in very centralised societies (priest/king autocracy), or under their influence. I have come to the conclusion that the early development of Aegean civilisation did not come about in this way, although by the great Palace periods in Crete and Mycenae centralisation was certainly a basic factor in the economy.

In the discussion which follows, the term "urbanisation" is used in a different sense from "civilisation." Both are intended to contrast with the essentially self-sufficient life of the earlier Neolithic village communities of Europe, such as the Danubian I settlements or the farmers of the Sesklo culture of Thessaly. "Civilisation" is used in the now-conventional way—implying, as in Clyde Kluckhohn's definition, at least two of the three following features: writing, ceremonial or religious centres, and settlements with a population greater than 5,000. If palaces are considered as representing ceremonial centres, one may properly speak of a Minoan-Mycenaean civilisation using this definition, even though settlement size was not large. The idea of civilisation without cities is likewise widely accepted for the Early Dynastic kingdom of Egypt and the Maya civilisation. "Urbanisation," as used here, relates to towns as "civilisation" generally relates to cities. In the 3rd millennium, Aegean towns like Troy or Phylakopi were not necessarily larger than the earlier Neolithic villages. Most of them, however, had stone-built houses, fortifications, a flourishing metal technology, some evidence of social stratification, and several new indications of wealth. In this sense they are towns, and we may speak of "urbanisation" in the absence of any new or more acceptable word. Those to whom "urbanisation" automatically implies the great cities of the Near East would perhaps prefer the terms "proto-urban" and "proto-urbanisation" for a settlement like Troy II, which was only about two acres (one hectare) in area. In such a case it will be sufficient to substitute "proto-urban" for "urban" in what follows: it is simply a question of definition of terms.

Urbanisation in the Aegean seems to have been the result neither of the development of a centralised autocracy, nor of significant influences from outside. It is my suggestion that a sudden and marked increase in trade was the principal causal factor, bringing in its wake new wealth, new craft specialisations, new weapons and defensive needs, and increased communication between maritime settlements in the Aegean. The

transformation of a village subsistence economy into an urban society took place very rapidly, and there is no evidence of a marked increase in population at the same time (although a population increase before this transformation, at the very beginning of the Early Bronze Age, seems possible).

In the outline which follows, an attempt is made to explain this transformation in terms of culture process, notably the expansion of trade. It is suggested that neither migration nor diffusion from outside the Aegean was a significant factor. Nor would multilineal evolution be a valid explanation, for the process of urbanisation in the Aegean was evidently different in kind from the process in the Near East, where irrigation and very high population density were important factors, rather than an independent example of the same basic process.

During the Neolithic period in the Aegean, up to about 3000 B.C., the largest and most prosperous settlements appear to have been the tell sites of the flat alluvial plains, especially of Thessaly, Macedonia, and Boeotia. Numerous Neolithic sites are known from southern Greece, Crete, and the Cycladic islands, but with the exception of Knossos they do not compare in size or apparent prosperity with those further north. The artefactual assemblages of these sites—Sesklo and Dimini are the most famous—include pottery, both plain and painted, chipped stone, stone axes and other tools, a few stone bowls, figurines, spindle whorls and "stamp seals" of clay, and bone tools (Weinberg 1965a). Obsidian from Melos was the chief known trading commodity (Renfrew, Cann, and Dixon 1965). Ceramic similarities between sites are close enough to indicate some transmission of ideas, but until characterisation studies have been undertaken, it will not be possible to assert that pottery was actually traded between communities. Essentially these appear to have been independent farming settlements, with obsidian as the only indication of, or motive for, contact.

The Early Bronze Age sees the development of a new settlement pattern. The increase in the number of settlements in Crete, the Cyclades, and southern Greece has been correlated with a supposed influx of population at this time, a theory for which there seems little evidence (Renfrew 1964). Very possibly it is to be explained rather by the development of Mediterranean polyculture farming, based on the olive and vine as well as the Neolithic cereals, such as is practised in the same areas today. From the present point of view, however, it is not so much the population distribution which is significant as the development in culture. By the end of the Early Bronze Age,⁸ the Aegean cultures of Crete, the Cyclades, southern Greece, and western Anatolia are on the path to civilisation. (Neither elaborate palaces with frescoes, drainage, and other advanced features nor writing appear, however, until the Middle Bronze Age, so it would be wrong to speak of a full civilisation at this time.) Admittedly the settlements are not very much larger than those of the Neolithic period; but in the Aegean Late Bronze Age civilisations themselves, the settlements are still exceedingly small compared with the Sumerian cities. If settlement size alone were the criterion, we could not speak of a Minoan or a Mycenaean civilisation.

⁸ A brief survey with a good bibliography is given by Weinberg (1965a).

The emergent factor in the Aegean Early Bronze Age, of the later 3rd millennium B.C., is an increase in the *intensity* of the cultural life—in the variety and richness of the artefacts found and in the sophistication of the crafts which they imply. Stone-built fortifications are seen at a large number of sites, including Troy, Chalandriani, Lerna, and so forth. Metallurgy becomes widespread, with the manufacture of tools, weapons, personal adornments, and luxury goods (gold and silver vessels). Personal ownership may be indicated by the use of seals in Crete and possibly by the discovery of clay sealings in Lerna. The beautiful stone sculptures of the Cycladic islands reach 1.5 m. in height, and stone bowls are common there and in Crete. A taste for luxury materials develops, especially in Crete (ivory, coloured stones), and the dead are buried with care and accompanied with rich personal goods either in cist graves (the Cyclades) or in communal built tombs (Crete). The much wider range of artefacts is what separates these cultures of the Early Bronze Age from those of the Neolithic, whose way of life appears to have remained essentially unchanged over 3,000 years.⁹ This richness links them to the civilisations of the Late Bronze Age and leads me to think in terms of *urbanisation* at this time, although their small towns (Troy, Phylakopi) are not necessarily larger than the Neolithic villages.

Although in the Near East, and to some extent in the Americas, it has been common to link irrigation with the development of civilisation, it does not follow that an increase in the efficiency of food production is an essential component of such development. The rather straightforward view that increase in leisure, afforded by greater agricultural efficiency, allows specialisation and leads to civilisation is not universally true. Many primitive communities have ample leisure, and agricultural efficiency is only one of the basic components of civilisation. More important are new needs, the trades to fulfil them, and the concomitant development in organisation. It is here suggested that these can lead towards civilisation without either a newly increased efficiency in food production or an increase in population.

The new needs here cited are documented by the development and ubiquity of the crafts and occupations of prehistoric societies which do not directly facilitate the food quest. These include town planning, palaces and temples, decorative metallurgy and ceramics, and indeed most of the manifestations of urbanisation and civilisation. In Crete and certain other early civilisations, these are accompanied by an increasingly centralised social organisation, but neither by irrigation agriculture nor by a striking increase in population. I suggest indeed that urbanisation and civilisation are not to be gauged by population increase or improved farming efficiency, since efficient and improved subsistence is presumably a goal of all human societies. Urbanisation and civilisation represent an increase in the *range* of environmental factors over which man exercises control. The most important of these environmental factors is man himself, and it is no coincidence that most civilisations show not only complex social organisation but also an elaborate procedure to regulate the relationship between man and

the gods, both before and after death. It is easy to overlook that what seems pointless superstition today was then a very practical concern; religion was a complicated and expensive technology designed to control man's environment or to influence it favourably.

All this is relevant here because there is a very close correlation between this "culture intensification" in the Aegean, the inception of metallurgy, and the development of trade. Most of the new features indicated appear in the Early Bronze 2 period, not in the Early Bronze 1, throughout the Aegean. So does metallurgy. So does a wide network of trade: Early Helladic pottery is found in the Cyclades and western Anatolia, Anatolian two-handled "depas" cups in southern Greece and the Cyclades, and Cycladic marble folded-arm figurines in southern Greece and Crete.

For reasons presented elsewhere (Renfrew 1967b), I believe that metallurgy may have originated independently in the Aegean; but whether or not the basic technique (of smelting and casting copper) was local is irrelevant, since its development (alloying to make bronze, tool types, weapon types) certainly was. The development seems to have been a rapid one in all the regions around 2500 B.C. The correlation between the metallurgical development and the growth of trade is striking. The explanation is, I believe, that for the first time there was a commodity worth trading, and on a large scale. For the first time trading became profitable.

The obsidian trade may never have been very significant economically, although culturally it did bring about contact. The quantities involved are small. With the appearance of a material which could transform crafts (as shown by flat and shaft-hole axes) and warfare (witness many daggers and spearheads), the situation changed dramatically. Perhaps the finished objects were traded first from a few more advanced metallurgical centres, such as Thermi and Chalandriani. Local metallurgical industries certainly grew up, however, and supply of the raw material must have been a pressing concern, although not until the Late Bronze Age is it documented by finds of ingots.

In the Early Bronze 2 period, therefore, the local development of metallurgy must have set up a demand throughout the Aegean for metal artefacts, a demand which could not always be satisfied locally in each region. Trade for the first time played a major economic role. This increase in trade and in contact produced, and is reflected by, the widespread distribution in the Aegean of recognisably local products. The ceramic types for the first time begin to follow metal prototypes. Unfortunately, the place of origin of these traded objects has not been established by any characterisation study (a study of marble has given negative conclusions), but the Anatolian inception of the "depas" cup and the Cycladic one of the folded-arm figurine is universally accepted.

Metal was an obvious form of wealth, more notably so than stone in Neolithic times, and the development of fortification may be related to this and to the increase in seafaring. In any case, the intensification of culture at this time has as two causative factors the economic upsurge produced by the invention and production of a

⁹ For a general account, see Caskey (1964), Vermeule (1964), and Pendlebury (1939).

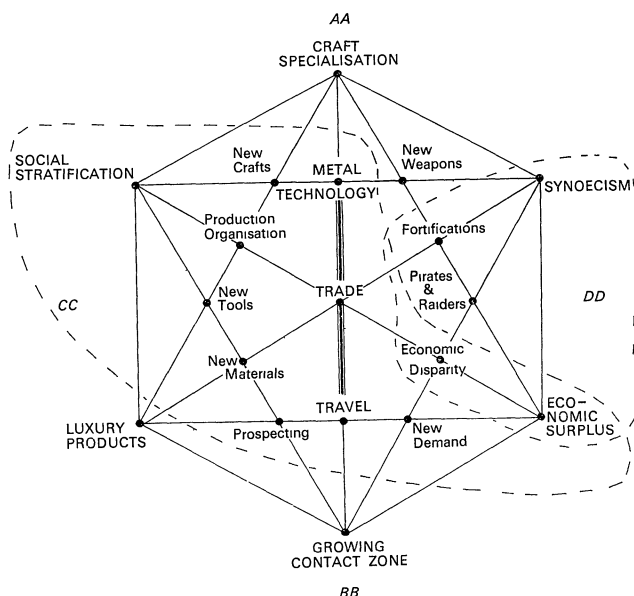


FIG. 2. Urbanisation in the Aegean: the interrelationship of some of the new factors generated by the inception of metal-lurgy and the expansion of trade in the 3rd millennium B.C. Alternatively, the points may be regarded as elements within severally mutually overlapping *sets* (delineated by broken lines): Set AA: industrial system; Set BB: communications system; Set CC: non-agricultural wealth; Set DD: defence.

new commodity and the striking increase in communications within the Aegean. These were certainly more important than any increase in agricultural efficiency and population¹⁰ brought about by the introduction of metal tools.

It would obviously be desirable to set up this explanation in quantitative terms, although this would be difficult at present. Clearly, too, the various systems at work require closer definition: trade may be conceived as constituting one economic system within a single community, within a given culture (or "tradition"), or within the whole of the Aegean contact zone (or "interaction sphere"). New concepts are required, for we are no longer dealing with essentially subsistence economies where the functioning of a system can be viewed as an almost automatic process depending on feedback from a very limited number of factors. Society in transition

¹⁰ The idea is contrary to the views of many culture historians, such as Childe and White (see White 1959: 293).

Abstract

This paper sets out to re-evaluate one of the chief processes leading towards culture change in prehistory. Widespread diffusion has for too long been the guiding principle in European prehistory; its validity is questioned here. The study of trade (defined in the first instance as the exchange or traffic of materials), and specifically the scientific analysis of raw materials, suggests that the importance of migration and of diffusion in European prehistory have been exaggerated. Change has often been due rather to the operation of local factors.

The quantitative study of economic systems, and especially trading systems, for an understanding of the

from subsistence community to urban settlement has reached a stage of complexity in which the variables are inconveniently numerous. New ways of handling these will have to be devised. The simplified (and perhaps naively primitive) representation of the network of interrelated factors given in Figure 2 at least indicates that several forces are now relevant which were not so at an earlier stage. Trade is only one of these, but obviously a crucial one. The factors listed are those which can be documented for the later-3rd-millennium Aegean on the basis of the archaeological evidence. To this extent—despite the somewhat cabalistic appearance of the diagram—they are not hypothetical, although other valid factors could perhaps be found. Although constructed for the Aegean, the diagram suggests applications in other regions. There remains the need, however, to structure the variables in a more coherent and less arbitrary way, and until this can be done the explanation will remain a rather rudimentary and descriptive one.

Many of these factors, of course, are implicit in the further development to city life and civilisation. There one expects to find, in addition to increased community size, evidence of some centralised or civic direction facilitated by the use of writing for commercial purposes. The media of public and artistic expression are correspondingly more impressive in scale. The urban communities of the Aegean Early Bronze Age may be regarded as intermediate between the village communities of the Neolithic and the Minoan-Mycenaean civilisation, and they show, at an early stage of development, many of the new economic systems on which the latter depended.

If such an explanation be adopted for urbanisation, and indeed for civilisation in the Aegean, the need for significant influence from "higher cultures" in Egypt or Sumer, let alone any invasion of civilised or skilled migrants, disappears. It is my belief that a similar pattern of events will emerge in Iberia when the chronology of the Copper and Bronze Ages there is better understood (Renfrew 1967a). There is probably no need, and certainly little evidence, to link the development of urban centres, such as Los Millares or Vila Nova de São Pedro, with civilising influences from the Aegean.

"Savages," said Lord Raglan, "never invent or discover anything." I am sure, on the contrary, that they are inventing all the time, and that trade is one of the key factors enabling these inventions to contribute to economic progress and cultural development.

culture process at work is advocated. It is exemplified by the obsidian trade in the Near East in the 7th and 6th millennia B.C.

Finally, an attempt is made to explain the formation of urban communities (i.e., of small townships) in the Aegean in the 3rd millennium B.C. as the result of local cultural processes, without significant outside influence. The chief of these processes was trade, necessitated by the rise of metal technology. Urban settlement arose in the Aegean without irrigation, without a significant rise in population, and without a high degree of centralisation. Neither a diffusionist explanation nor an appeal to multilineal evolution provide an adequate framework for its explanation.

by ELIZABETH CHESLEY BAITY*

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Renfrew's article interested me greatly, not only for his brilliant defense of the hypothesis that the importance of migration and diffusion as processes leading towards culture change in European prehistory has been exaggerated, but also for his exposé of new methods in the quantitative study of archaeological materials. His study of Cycladic metallurgy in the Aegean Early Bronze Age and his arguments against the theory of substantial colonization from the Eastern Mediterranean in Bronze Age Iberia (Renfrew 1967*a, b*), like the present article, are surely destined to strongly influence young archaeologists.

It is evident that problems of prehistoric migrations can be solved only by such intensive localized studies, which question previous unexamined assumptions and initiate new types of research. There is some danger, however, that Renfrew's views may be carried to undesirable extremes by less critically minded followers, to the extent that speculative thinking would be discouraged with regard to areas where the archaeological evidence is as yet too scanty for quantitative analysis. My comments, made on a basis of an agreement with Renfrew on so many points that it would be tedious to enumerate them, will therefore focus on qualifications which it appears to me desirable to keep in mind.

First, it might be helpful if Renfrew would define more closely his idea of that point at which expanding trade inevitably becomes an agent of diffusion with resulting culture change: this appears to be a problem especially with regard to sophisticated metal products such as improved weapons and tools, with their high potential for causing rapid social change, some of it due to the impact of customs and cults which appear to have accompanied early metallurgy. (Perhaps we can now replace "Galton's problem" of discriminating so-called historical from functional associations in cross-cultural surveys [Naroll and D'Andrade 1963] with this, as "Renfrew's problem.")

In various other cases "superior knowledge" necessarily traveled along with the objects transferred. In this connection one remembers not only the Neolithic magical practices that accompanied seeds and stocks, but also the horse-trainers who went along with war-chariots and horses when these were first acquired by Western Asian princes and who now and then formed a new elite. One thinks also of the social effect of the exchange of

princesses as royal wives. These women, traveling with their entourages and cherished gods and cults, surely also carried new ideas which must have affected their descendants if not their husbands.

In certain cases the mechanisms of trade must have incurred culture changes. Renfrew has referred to the *karums* of the Old Assyrian merchants in Anatolia. These and the Indus and Mesopotamian colonies of foreign merchants, every day more evident archaeologically, certainly must have occasioned social change, especially after the destruction of the Indus cities, when some of the orphaned colonies in the West must have been absorbed into the surrounding cultures. When Starr (1941) first inferred these merchant colonies on the basis of stylistic studies of painted pottery motifs along a continuum from Tell Halaf to Harappa, the other archaeological evidence since reviewed by Mallowan (1965) had not yet been discovered, nor had Kramer (1963, 1963-64) and other Sumerian scholars yet searched for and found the textual evidence which now links the two areas.

The example of the Indus colonies and the Old Assyrian *karums* suggests another minor point to be kept in mind; early trade colonists appear to have differed considerably in the extent to which they used home products which can be archaeologically identified. Renfrew has of course noted that "factors of preservation make an obvious difficulty in quantitative studies." Mycenaean colonies in Western Asia can be identified by characteristic sherds and other artifacts, but Özgüç (1963) has stated that had it not been for the baked clay tablets with cuneiform inscriptions found in ovens at Kültepe, the presence of this Old Assyrian colony might have gone unobserved, as the colonists used only local furnishings. Judging from the small size of their ships, Assyrians, Phoenicians, and almost certainly Indus traders traveled light. That they traveled extensively is indicated by a Phoenician inscription found in Brazil (the validity of which has long been disputed but was recently affirmed on the basis of new evidence by Cyrus Gordon), which speaks of a ship blown off its course while rounding Africa—a type of event that must have occasioned much inadvertent diffusion in the early days of sailing ships.

Several problems converge with regard to the quantitative study of early trade in the Persian Gulf and the Indian Ocean: first, these protohistoric Asian traders not only carried perishable products such as textiles, foods, dyes, incense, etc., but may also have kept records on perishable materials including paper, papyrus (cf. Wild 1966), palm-

leaf, textiles, or skin; secondly, in addition to the inevitably discontinuous evidence of sea trading, the early settlements on offshore islands may have disappeared as a result of repeated devastations such as that which recently ravaged Kharg Island. This is suggested by the appearance on ancient maps of many offshore islands where few or none now exist. Hapgood's (1966) study of the Peri Re's map assembles much evidence for this possibility.

It is of course inadmissible to invoke the argument of "lost evidence"—except as a reminder that some of the problems of explication of the past, which must form the basis for explanation of cultural processes, cannot be resolved until more of the evidence is in hand, and that not all of the evidence can be quantitatively assessed. Until the legendary Tartessos area is found and archaeologically investigated, the question of exotic contacts which may have affected social developments in the Iberian Bronze Age cannot be entirely ruled out, though I, like Renfrew, believe that the Iberian Peninsula may have been a center of origin. Certain astronomically determined rituals may also have spread eastward along the Ibero-Saharan continuum (Baity 1968:103-42). My point is that it is too early to dismiss the cumulative effect of much that cannot now be studied quantitatively, such as similarities between Iberian, Asian, and African rock art motifs, ethnic and place names, and fire, bull, and solstice rituals. The appearance of zebu in African rock art indicates at least three separate eras of importation of Asian cattle into Africa. Turbans and beards in Saharan rock art are incontestably Asian (unless one invokes a current flowing the other direction) even to the detail of sprigs in the turbans, such as those of sprouted grain still worn in the renewal rituals of certain aboriginal Indian tribes.

For all the inconclusiveness of stylistic comparisons and of interpretations of rock art, oral traditions, rituals, ancient texts, maps, and travelers' accounts, some of this evidence may indicate early trade that cannot yet be archaeologically attested but that surely affected cultural processes: this is particularly the case with the ancient "Sealands" of Arabia, attested in Mesopotamian records, which may have been the locus of trade involving perhaps the Makran Coast and East Africa (cf. Dougherty 1932). Renfrew has noted the difficulties in establishing facts about some of the trade exchanges; obviously bones, obsidian, ceramics, glass, and metal afford more scope for quantitative studies than do fine textiles, furs, incense, gums, waxes, spices, salt, slaves, grain, rice, onions, and other

such exports. Some of these perishable trade goods are listed in early trade records. That Indus traders carried such goods in bundles is indicated by the numerous seals and sealings found; they also were interested in goods with low bulk and high value, such as pearls and precious stones, which would have been carried away by refugees and seldom abandoned. The Sudan's ancient trade in salt and slaves is another case in point. Researchers using the types of tools evolved by Renfrew, Gardin (1957), and the Binfords (1964, 1965) will doubtless develop methods for studying these elusive elements of early trade.

In addition to these obvious losses from the archaeological record due to the ravages of time are those losses which may yet be recovered. New World metal-working centers known to have existed have not yet been discovered (Easby 1966). Close similarities between African and Indian or Indonesian musical instruments, scales and tunes (Jones 1960) and between the tie-dyeing and bronze-casting techniques of these areas may yet be explainable archaeologically apart from the relatively late Indonesian trade: Fagg's discovery of Asian tools in protohistoric tin-workings may foreshadow future finds confirming the Indian tradition of East Africa as "the third India."

Renfrew and others will undoubtedly find ways of developing classificatory criteria for such types of evidence; here, too, intensive studies of various subjects, motifs, and systems in isolation is indicated. It appears likely that the Ibero-Saharan continuum, not yet adequately explored, will furnish the data for new studies of the problem of tele-commerce and cultural process. Certainly in the final analysis the postulated protohistoric African trading systems, like those Asian systems to which Childe attributed the economic development of prehistoric Europe, must, as Renfrew rightly insists, be proven rather than surmised; but at present these categories of cumulative but insubstantial evidence may serve as indicators of likely areas for search by interdisciplinary methods which may involve textual scholarship and astro-archaeology as well as archaeological techniques.

by D. K. BHATTACHARYA*

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The appearance of certain distinctly foreign cultural elements in a given industry has been a problem for prehistorians from the beginning. Various theories have been proposed to explain such a situation. The most popular of these is what may be called the contact or intrusion theory. Trade, in Renfrew's restricted use of the term, is only an

agency through which such contact or intrusion is likely to occur. Renfrew's is not the first attempt at understanding these phenomena within their total cultural and organizational context. It is, nevertheless, a commendable piece of work. I should like to mention here only a few points at which I feel uncomfortable about it.

1. It seems to me risky, or misleading, to consider the decrease in the proportion of obsidian with increasing distance from the 100% or supply zones as an index of the existence of trade. The concept of distance involves two considerations: transport and the technical capacity to break down various natural barriers. Again, the kinds of barriers must play an important role. Where transport is efficient, a large area may show 100% obsidian from one distant (real) supply zone.

2. When population movements over large as well as small areas can be demonstrated, do we still use the word "trade" to describe the transport of obsidian? We do not really know, for instance, that a community from the supply zone did not physically migrate to a zone in which there was less obsidian and eventually turn to local materials when their original stock of obsidian was exhausted. Over a period of time, this would certainly result in a lesser proportion of obsidian at the migrant sites than at sites in the supply zone. Most important prerequisites to Renfrew's thesis, then, are the establishment of the relative ages of the sites to be compared and the comparison of their skeletal material. Both of these constitute important practical problems. It seems to me that they are especially important for the Neolithic and early Bronze Age, when people were fairly mobile.

3. It is a mistake, though one that is difficult to avoid, to try to explain the processes of the past in terms of the reasons and facts of the present. While we may be very critical of the evolutionist model of culture change, we are unintentionally using evolutionist arguments in a limited sense when we extend modern phenomena to prehistoric communities. If we try for a moment to release our thinking from the prison of our own culture, it is very easy to imagine groups migrating to the obsidian zones themselves rather than importing this material. Further, apathy towards change in relatively isolated cultures cannot altogether be ruled out. These considerations are not to be taken as disproof of what Renfrew has so convincingly shown here, but only as an indication that his thesis may not apply to every situation.

4. Renfrew's attempt to trace urbanization to trade, using the Aegean complex as his example, is a venture he

should not have undertaken. His argument against "centralised societies (priest/king autocracy)" as the cause of urbanization and in favor of "sudden and marked increase in trade" is not at all convincing. This sudden increase in trade presupposes a stage of peasant trade within an urbanized setting. One cannot imagine the development of such vigorous trade as to lead to urbanization without a centralized authoritarian society, however weak. It is safer to presume a simultaneous development of both these features, the one reinforcing the other. A community with weak trade would eventually be weak in control, while a community with strong centralized organization would perhaps make its weak trade strong and efficient.

by LEWIS R. BINFORD*

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This is an important and provocative paper. It questions the adequacy and validity of traditional arguments advanced by anthropologists (particularly archeologists) in treating the complex problem of the dynamics of culture change. Renfrew argues strongly for the use of improved methods of data collection and analysis; these are surely important points, and this paper is a major contribution in pointing up the necessity for achieving those aims.

Renfrew argues that in instances of the initial appearance of urban socio-cultural systems, trade in various commodities historically precedes the appearance or urban development. This is certainly a sound empirical generalization and is supported by a large body of New World data. The significant recognition of the fact that trade systems often precede urbanism and the suggestion that the explanation of this phenomenon may take us a long way toward understanding the processes bringing about major cultural transformations are major contributions.

One problem, however, in Renfrew's discussion is an apparent confusion of terms—particularly "urbanism" and "civilization." This confusion is evident in statements like the following:

The much wider range of artefacts is what separates these cultures of the Early Bronze Age from those of the Neolithic. . . . This richness links them to the civilisations of the Late Bronze Age and leads me to think in terms of urbanisation at this time, although their small towns . . . are not necessarily larger than the Neolithic villages.

Characteristics generally cited as differentia of civilization are treated by Renfrew as indicative of urbanism, even though the appearance of cities is not demonstrable. Many workers, in dealing with analogous problems, have found it useful to distinguish between urbanism

and civilization. This distinction seems necessary, since many of the political and economic changes that normally indicate increases in sociocultural complexity of an order approaching civilizational levels occur without concomitant changes in demographic structure in the direction of increasing aggregation and the appearance of cities. In fact, the data cited by Renfrew may constitute an example of this type of change. His failure to distinguish between organizational phenomena (sociopolitical and economic changes) and distributional phenomena (settlement pattern, forms of demographic aggregates) makes it difficult to understand some of his discussion.

Several even more important problems arise in connection with Renfrew's attempt to explain the appearance of urbanism-civilization in terms of the causative role of trade. Renfrew concludes that the early development of Aegean civilization did not occur in the context of increasing centralization in sociopolitical organization. This conclusion is very strange, since he cites many facts of the archeological record from the Aegean that are generally considered good evidence for increasing organizational complexity and centralization of power. Renfrew cites the appearance of new needs and states that these were best met through trade; trade in turn had a catalytic effect in bringing on new levels of organizational complexity in the socio-cultural systems of the Aegean. This strikes me as a rather superficial reductionist argument. Where did the needs come from? Why did they appear when they did? Are they the summation of individual psychological preferences and inclinations? Can culture change be understood through an argument that cites psychological preferences and needs? I fear that it cannot. New needs (individually speaking) are generated in the context of institutional changes through which new statuses appear and because of which individuals are required to play new roles. The new needs relate to the social maintenance of these differentiated statuses and to the new self-images that grow out of the execution of new roles. It is these institutional changes that anthropologists should attempt to understand and explain. As Renfrew himself points out, more work is needed in the development of concepts and analytical methods.

Since the theoretical underpinnings of research are crucial in determining the kinds of questions we ask and the way in which we conceptualize the empirical world, critical examination of the assumptions underlying Renfrew's argument is very much in order.

I hope that these negative comments will not be generalized by the reader to

the entire article. Similarly, I hope that my comments will not be viewed as destructive with regard to Renfrew's general programmatic argument. My criticism is offered in light of Renfrew's recognition that his arguments are an initial step in the direction of more precise knowledge of cultural dynamics. His paper provides welcome relief from the sterile arguments of diffusionists and of advocates of mass migration as a way of explaining culture change. Renfrew advocates an ecological approach and analysis of social systems at the local level as profitable means of gaining understanding of cultural dynamics. I am in complete agreement with this approach.

by B. BRENTJES☆

Berlin, D.D.R. 6 1 69

Renfrew proceeds from the necessity of a critical re-examination of former theories in prehistory in the light of new evidence, and he deserves thanks for his questioning of earlier schemes. I have the impression, however, that his critique is a little too self-confident; it fails to consider the probability that his own point of view will one day be open to criticism that is as drastic as his own and that, like his, overshoots the mark. Childe's *The Dawn of European Civilisation* was in its time a meritorious attempt to gather all the known material into a single historical framework, replacing the existing mess of divergent theories. Renfrew sets up, in his "models," specific enemies, easy to overcome; but in reality they are little different from his own thesis. Thus his idea about and his argument against the evolutionist model is simple, but wrong. There are in reality general laws of evolution, and Renfrew recognizes this in his discussion of urbanization in the Aegean. He exaggerates the importance of these laws, however, by neglecting the fact that a change in an archaeological culture may also have a cause absolutely outside the society; the romanization of Gaul was not a result of Celtic development, and the destruction of the culture of the Aztecs came from abroad, not from within. I believe that for some periods in history the archaeological picture has been determined by great movements of people, though the world has never been a railway station crowded with wandering hordes and traders. The time of the diffusion of agriculture and husbandry was such an epoch. (In spite of Renfrew's scepticism, agriculture and husbandry did come from an Oriental centre to Europe.) Trade seems to have played an unimportant role, if any, during this time. It is still necessary to study the concrete forms of this diffusion; but inasmuch as

we do not know exactly what they were, the much-criticized "diffusionist model" is useful because of its indefiniteness. In view of Renfrew's polemics against the idea of Sumerian prospectors in Europe (a theory I do not share in the form given by Renfrew), it is not without piquancy that the earliest evidence of Oriental trade in Europe (and of real trade at all) takes the form of three Sumerian tablets found in the early Vinča levels at Tartaria, Rumania (Milojević 1965), showing Mesopotamian influences upon the Southeast European early Neolithic.

There are in fact periods to which Renfrew's thesis is appropriate—in which internal, local developments are decisive and trade is the main form of exchange—for example, the Bronze Age, discussed by Renfrew. The period of the nomadic wanderings of the horse-breeders of the Eurasian steppes, from the Scythians to the Mongolians, is an essentially different case; here internal factors are the prerequisites, external ones the triggers.

In conclusion, Renfrew's arguments are acceptable insofar as they lead to a critical re-evaluation of old theories, but they are one-sided generalizations from a single period of history. In a critical analysis of trade in European prehistory, such important finds as the Tartaria tablets cannot be ignored even though they do not fit the theory.

by GEORGE DALTON☆

Evanston, Ill., U.S.A. 15 XII 68

I should like to enlarge upon one of the points Colin Renfrew makes in his intelligent and clear paper. It is all too often true that archeologists cannot establish with certainty what the forms and functions of external trade were on the basis of their archeological findings alone. I should think, therefore, they would welcome the second-best solution of specifying the small range of plausible possibilities, on the basis of anthropological analogies. If the prehistoric societies archeologists unearth had the same kinds of internal organization and technology and faced the same external environmental conditions as the societies described by social anthropologists until quite recently, it should not surprise us if the forms and functions of foreign trade were also similar and the items traded of the same order—principally luxury, elite, military, or prestige goods. The risks to physical safety of long-distance travel were equally great (which means that the items sought were highly prized, and the trading expedition was protected either by its own military strength, as with the Vikings, or by the emblem of political authority, or, as with Malinowski's *kula*, by host-friends in the territory).

I think it can be argued persuasively from ethnographic evidence that the modes of transaction employed in external trade by pre-industrial societies were very few and varied systematically with the character of internal social and economic organization. Early foreign-trade transactions were variants of three basic modes: ordinary market or commercial trade, gift trade, or politically administered state trade. Where the two trading parties represented radically different socioeconomic systems, special trade devices, such as silent trade and ports of trade, were contrived. Renfrew is right to say that "the interior workings of the society, particularly its economic subsystems, have to be understood" in order to understand external trade. But the striking similarities between prehistoric communities and those studied by social anthropologists in modes of transaction, the range of goods traded, and level of technology—techniques of transport as well as techniques of production—suggest that their internal socioeconomic organization must have been quite similar as well. The ethnographic literature of social anthropology and the analytical literature of economic anthropology can suggest fruitful analogies to archeologists wanting to establish the range of internal social organization compatible with the archeological evidence of foreign trade.

by CARL-AXEL MOBERG☆

Göteborg, Sweden. 15 XII 68

Renfrew's article represents a most interesting approach to problems of central importance. It cannot be denied that he has good reason to argue that "much" thinking in this area is outdated and that "too much" emphasis is placed on inadequate models for cultural change. "Much" is not "all," however. One might mention such attempts as that of Malmer (1962) to replace the invasionist and "aquatic diffusionist" models for explanation of the Neolithic "Battle Axe Culture" phenomenon with a model of change in religious system; or Stjernquist's (1967*a, b*) work on the *ciste a cordoni*, which studies the function of a system of trade as decisive *inside* the larger framework of a diffusion model. Further, one should not forget the mainly indirect but lasting importance for the prehistory of Scandinavia of Bolin's numismatic historical investigations (1926) on the trade systems of the Roman and Migration Periods.

A dichotomy between "European Prehistory" and "a recent development in American archaeology" (p. 153) should not be exaggerated (Gjessing 1960).

Next, two remarks on investigation of specific trading systems: (1) A Scandina-

vian archaeologist would add flint to the list of items of particular interest (Becker 1959). (2) Renfrew's emphasis (cf. also Ankner *et al.*, 1965; Ankner 1968; Beck 1968) on the need for better knowledge of the basis of the amber trade concept is very welcome in a region where for some time there has been doubt about it (Moberg 1955; Brøndsted 1958; Stjernquist 1967*b*: 31–34).

Finally, a question on Renfrew's Figure 2: to the "new" factors shown ("New Demand," "New Materials," "New Tools," "New Crafts," and "New Weapons"), would it be incompatible with the author's theoretical framework to add "New Ideas"?

by PHILIP E. L. SMITH☆

Montreal, Canada. 10 I 69

Renfrew's article strikes me as a good presentation of one of the approaches which is transforming some aspects of archaeological interpretation today. He has for the most part presented his case clearly and convincingly, to my mind. There does seem to be one small inconsistency in his argument concerning the Aegean, if I am reading him properly. He repeatedly states that 3rd-millennium urbanization in the Aegean developed without significant outside influences, yet the evidence he mentions elsewhere (metallurgy, new craft specializations, new weapons, seals, etc.) does imply significant outside influences. I know what he is getting at and on the whole agree with his argument that these were mainly the consequences of an internal impetus rather than of preponderant outside influence; but it is not too well expressed in this example.

Obviously, as he emphasizes, the factors responsible for cultural changes differ from one region to another. I find particularly interesting one aspect which he refers to only obliquely as being probably operative in the Middle East: high population density (p. 158). For several years T. Cuyler Young, Jr., and I have been exploring a hypothesis according to which the dynamics of population size and change and other dependent and independent variables (especially agricultural technology and productivity) might explain a large part of the events between the close of the Pleistocene and the 1st millennium B.C. that we have been investigating in the field in Iran. Just as Renfrew does in the Aegean, we find that the events can be better explained in terms of local cultural processes than as results of extensive outside influences. I do not want to anticipate here the results of this unfinished work, but I find it interesting and even encouraging that Renfrew's thinking follows somewhat the same contours, although in this paper the

factors he sees as important are not the same as ours.

I gather from his references on p. 159 that he is not inclined to see population increase as very significant in bringing about Mediterranean urbanization. If this is so, then it is one more indication that no single model is applicable for differing regions and environments. Nevertheless, I suspect that the population increase he admits is possible just *before* the transformation (p. 158) is more important than he seems to believe. Finally, I agree with his suggestion that the idea of leisure plus increased agricultural productivity leading to specialization and urbanization has often been misused by archaeologists. Insofar as it is possible at all to detect a cause-effect relationship, I think it more likely that the total leisure of a society was reduced, not increased, with the advent of more complex societies.

by JULIAN H. STEWARD

Urbana, Ill., U.S.A. 12 XII 68

In view of Renfrew's unfortunate misrepresentation of cultural evolution, he will doubtless be appalled that I consider the present essay a rather good exemplification of one possible line of evolution. To some, evolution is a model; or, worse, it is an *a priori* scheme of developmental taxonomy. To me, it has always been an empirical search for causes or explanations of the evolutionary transformations of sociocultural systems. Consequently, I have always doubted the utility of diffusion as an explanation; and I entirely sympathize with Renfrew's approach, even though some particulars are not wholly convincing.

Since I have long been classed as an evolutionist, a position that has been frequently misunderstood, Renfrew's cavalier dismissal of the approach affords an occasion to set the record straight. In my article of 1949 on "Cultural Causality and Law; A Trial Formulation of Early Civilizations," I did not consider myself an evolutionist. The hypothesis then presented concerned a single case among an undetermined number of evolutionary lines that I subsequently called multilinear evolution in the Wenner-Gren World Conference of 1952. It was not a universal explanation, and it did not advocate limitation to the superorganic. (Although evolution is a useful term, I have been inclined to disavow it [Steward 1967] because of the rigidity with which it is commonly used.)

Causality is still viewed with doubt, but today this is because of the multiplicity of interacting causes rather than because interest is restricted to cultural historical reconstructions. Provisional explanatory formulations based on what

seems to be relevant in descriptive reconstructions are necessary, and it is rather shocking that Renfrew has so failed to grasp their scientific implications that he should have called such formulations tautologies.

I cannot cite here all the relevant literature, but profound modifications have been made in my earlier hypotheses. The initial explanation of early civilized states was Wittfogel's irrigation hypothesis, but this was modified years ago following Robert Adams' work in Mesopotamia. Although large-scale irrigation was crucial in areas of deeply entrenched rivers such as the Andes, it had secondary importance in certain others. Adams has since demonstrated close parallels in the evolution of urban societies in Meso-America and Mesopotamia, where the interacting causal factors were specialized production in closely adjacent microenvironments, trade, and control of trade by the pre-existing priesthood. Other factors had varying importance in different areas, but a cross-cultural model of this general variety goes far as a provisional explanation of many, but obviously not all, early civilized states.

Trade in the Aegean may have stimulated the use of certain rare items without a large population or the development of centralized controls, as among many far more primitive societies. But the Aegean did have agriculture, permanent settlements, obviously a class system, and therefore a social segment for which the craft products were largely intended.

I would therefore suggest to Renfrew that his Aegean case has many characteristics of the chiefdoms of the northern Andes, parts of Central America, and doubtless elsewhere, where a central authority, although certainly not the agency that originated trade, became closely associated with its products. An additional line of multilineal evolution that is possibly discernible cross-culturally is suggested. Comparisons would serve to assess the roles of such obviously interacting factors as specialized food production whether in contiguous areas or in more distant locales; transportation; surpluses; population growth and aggregation; a class system, including rulers of special kinds, creation of craft specialties, monumental architecture, and other products for state consumption; and additional factors. As the larger correlations that Renfrew dismisses as tautologies can be broken down into smaller, more detailed processes, explanatory formulations will become more firmly established; but it would be a grievous error to suppose that the local presence of unique phenomena invalidates other causal relationships.

by SAUL S. WEINBERG☆

Columbia, Mo., U.S.A. 15 1 69

Renfrew proceeds from broad statements on cultural change to narrower considerations of trading systems and, finally, trade and urbanization. I should like to comment in the reverse order. Renfrew and his colleagues have done pioneering work of the greatest importance in the analysis of obsidian, which now permits tracing different kinds of obsidian to their sources with a considerable degree of certainty. The quantitative study reported here gives an idea of the amount of obsidian being transported from the source to sites in the Zagros area and indicates a primary area of distribution, as well as a secondary area farther afield to which much smaller quantities of obsidian were going. Thus a reliable pattern of distribution has been established for one kind of raw material and the way pointed for similar analyses of other materials.

In turning from such basic analyses to their implications for patterns of culture change, Renfrew gets into a more speculative area. There is first the question of whether or not exploitation of sources of raw material always involves trade. By definition, "All trade is a two-way process" (Renfrew, p. 154). I question whether the exploitation of Melian obsidian sources before, say, 3000 B.C. was such a process. The appearance of obsidian on the Greek mainland is now attested from mid-Mesolithic levels at the Franchthi Cave in the southern Argolid (T. Jacobsen, report circulated privately); ten years ago Theochares (1959) had already noted obsidian microliths, presumably of Mesolithic date, on the island of Skyros. From then on, obsidian was in ever increasing use in the Aegean. Melos, however, shows no signs of habitation until about 3000 B.C. or a little earlier. There is here no indication of trade, no sign that those who traveled to Melos to obtain obsidian were bringing anything with them or were "paying" anything for the raw material they took from the island. Nor could they acquire any cultural traits from this apparently empty island. What is clearly demonstrated is that there was seaborne traffic probably as early as 8000 B.C., possibly earlier, and this is of significance for culture change.

Granted that trade, or movements of peoples for the purpose either of obtaining raw materials or of carrying finished products from one place to another, had very early beginnings and increased in volume over several millennia, the travel involved and the resultant human contacts cannot but

have had important consequences for cultural development and change. Renfrew has helped considerably to define more exactly the nature and degree of some trade. In his evaluation of the role of trade in culture change, however, he makes some rather sweeping statements which in many instances are as untenable as the generalizations he is trying to eradicate. True, trade is a form of contact for which material evidence often remains down to the present; but it is not the only form of contact witnessed by surviving material remains, the stuff with which archaeology deals. Changes in settlement patterns, especially large-scale abandonment of sites, extension of occupation to new sites, or both occurring at the same time; changes in architecture, particularly drastic changes in orientation in successive strata; changes in tomb types and burial practices; and precipitous appearance or disappearance, or both, of pottery fabrics, figurine types, seal forms, or any of a number of other types of artifacts have often been cited, individually or collectively, as indicators of cultural change due to a variety of forms of contact: destruction through invasion, dislocation through migration, diffusion of ideas through human contact (trade being one important form). There has recently been a spate of studies showing why each of these types of change separately, or even certain combinations of them, cannot any longer be considered as documenting invasion, migration, or any other form of change involving the movement of people. All these arguments seem good; all will be eagerly seized upon by highly nationalistic elements in numerous nations, always trying to prove the autochthonous nature of the population in each country. On the other hand, it seems folly to argue that people were moving about for purposes of trade from Mesolithic times on and yet never moved because they wanted a better place to live, or the lands and possessions of their more fortunate neighbors, or waterways, or a warmer or wetter climate. To deny these motives is to deny that we are dealing with human beings. The question is how to document such population movements and their consequences from extant material remains. This may be more difficult than documenting some forms of trade, such as now has been done with obsidian; as a result, the importance of trade may be overemphasized. Progress in the analysis of trade may eventually define so well the role of trade as a factor in culture change that we shall know better what aspects of that change must be attributable to other factors. For the present, we must assume that such other factors existed and try, accordingly, to refine our tech-

niques to the point where they can be more easily recognized and documented; to deny them is antiprogressive, for then we shall see no need to study them.

I would urge that Renfrew, and others, stop flogging dead dogs. It has been a long time since "Minyan" ware has been thought sufficient evidence to document the "Minyan" migration (p. 153), but this does not deny the possibility of such a migration, or of some drastic cultural change in Greece around 2000 B.C. The fact that the change was much for the worse, that contacts with the outside world seem to have become considerably less with the change, may mean that Renfrew's techniques of studying trade will in this instance offer little by way of explanation. One would do well to remain open to many different possibilities for such a change, at the same time being wary of accepting any single explanation, or accepting one that can already be documented "scientifically" over others for which that is not yet possible. We are witnessing a fashion in archaeological interpretation which is capable of as much misuse as the earlier ones assailed in this and other similar articles. Trade cannot be accepted as the explanation for all, or even most, culture change, but we shall watch with interest and keen expectation the development of techniques that will allow the better documentation of ancient trade, to the ultimate end of understanding this one factor conducive to change, and so perhaps of understanding others as well.

by ROSCOE WILMETH☆

Ottawa, Canada. 10 XII 68

Renfrew has made some very useful suggestions for a more accurate assessment of the prehistoric importance of trade, especially his use of quantitative data to show changes in trade through time and his calculation of a "fall-off" point

beyond which trade material from a specific source suffers a sharp decline. Perhaps equally important is his questioning of generally accepted views of certain widespread trade networks in ancient times. The problem of reciprocal trade, to which Renfrew refers, will probably remain a tricky one, since the fact that a trade item such as obsidian may be transported as much as 300 km. does not mean that the goods for which it is traded at the end of the route must necessarily travel all the way back to the starting point. The situation will be further complicated if at some point in the system "hard goods" are traded for perishable items.

There are a number of points at which interpretation of the role of trade does not appear to be as clear-cut as Renfrew seems to make it. One of these is his distinction between trade within an area as opposed to diffusion from without, as a source of culture change. Renfrew regards the former as more important; but whether the dispersion of materials or manufactured articles is regarded as internal or external will obviously depend on how the area in question is defined and how its boundaries are drawn. Depending on the level of analysis, the population of a given geographical region may be regarded as a single cultural unit or as a number of related units. How the situation is characterized will thus depend on the orientation of the archaeologist.

Interpretation of the effect of the widespread trade network known to have existed in North America in classic Hopewell times may be cited as an example. Areas as far from the Illinois and Ohio Hopewell centers as the Gulf Coast and the Rocky Mountains were involved. One result of this traffic is the appearance in extreme western Missouri and eastern Kansas of settlements, some of them relatively large, yielding cera-

mics of general Hopewell character and other indications of contact with the East (Wedel 1943:15-61; 1959:542-49). The role of Hopewell contact in effecting a sharp break with the past in this area can hardly be denied. To regard this as internal trade within a single independent system, rather than as diffusion from one area to another, would require a definition of the area so broad as to be meaningless.

A more basic problem is that of evaluating the role of trade as opposed to other factors in cultural development. Renfrew notes, quite rightly, that trade cannot be assumed, but must be proved. It is equally true that when expansion of trade and accelerated cultural enrichment are shown to be contemporary, it cannot be assumed that the former is responsible for the latter; this also must be demonstrated. In the example Renfrew gives of the emergence of urbanization in the Aegean, I do not feel that his arguments are completely convincing. Although he does indeed point out a close correlation between "culture intensification," metallurgy, and trade, the demonstration of this correlation does not in itself provide us with explanations. Renfrew here regards the trade in metal as basic, but he does not provide details on sources and routes. In fact, his discussion here indicates a large degree of uncertainty.

The point is that it is extremely difficult to establish, as Renfrew attempts to do, the priority as a causal factor of one correlate over another. When in our own time the seemingly obvious implications of the correlation between cigarette smoking and cancer do not appear to have won universal acceptance, interpretations of similar correlations in prehistoric times are bound to be much harder. This should certainly not deter us, however, from making the attempt.

Reply

by COLIN RENFREW

I am grateful to all those who have commented on my article. In writing it, I was particularly conscious of the deficiencies some European prehistorians feel there are in our approach to the question of culture change. We are all, I think, groping towards better ways of describing what happened in the past, and "why" it happened.

It seems more and more evident that changes in a culture can often best be explained primarily in terms of factors and conditions *within* that culture rather than in terms of exterior influences, or even of illuminating comparisons with

independent developments elsewhere. And until outside influence can be documented it constitutes merely one explanatory factor out of several possible ones; its importance should not be exaggerated. Trade is another of these factors: to stress this, as I have tried to do, is not to deny that it often operates to bring in new ideas from other societies, nor indeed that societies can be overthrown or suffer acculturation from outside. Again, its importance should not be exaggerated, as Baity is right to indicate. Nor should trade be confused with simple travel to source to obtain a raw material. (This is relevant to Melian obsidian, as I pointed out in 1965 [Renfrew, Cann, and Dixon 1965:241] and as Weinberg emphasises: really effective trade did not

begin in the Aegean until the Early Bronze Age.)

To attack diffusionism may seem, as Weinberg puts it, "flogging dead dogs." But this dog just will not lie down: it has some bite in it yet! The invasion of "the Minyans" is, happily, little favoured today, but when dealing with earlier periods in Greece, Weinberg himself writes readily enough of "intrusive cultural elements" (Weinberg 1965b:296), "the arrival of . . . new cultural features" (p. 298), "a migration route over the Cyclades . . . another movement" (p. 302), and "the arrival of the first metal users on the Mainland" (p. 308). All these movements are perfectly possible and deserve the most serious consideration: I feel that some may be substan-

tiated by the evidence when this is presented in detail. But as they stand, these are claims of exactly the kind that I wished to question, based on sometimes vague parallels (megaron houses, black polished ware) and presented and accepted without an adequate consideration of the possibility of local evolution. Weinberg's surveys of Aegean prehistory are extremely valuable, and I do not wish to criticise except on this specific point. The following statement (Weinberg 1965b:308) does, however, appear both diffusionist in outlook and ill-substantiated in fact:

The Aegean was the recipient of repeated waves of migration from Anatolia and Syro-Cilicia in particular as well as of cultural influences that came independently of actual migrations. Much more sporadic were movements of people and cultural influences from the Balkans or beyond; the cultural movements from Greece northward, which have not concerned us here, seem to have been both steady and strong.

Is Weinberg's dead dog perhaps trying to run with the hare as well as with the hounds?

In highlighting my own dissatisfactions and points of divergence with some of the well-established approaches in prehistoric studies, I may have seemed lacking in appreciation, or even "cavalier." I should like, therefore, to redress the balance a little by emphasising the enormous contribution made by Childe; to take his work as a starting point, as nearly all European prehistorians do, is effectively—despite our wish to modify parts of it—the sincerest form of flattery. Next, the nettle proffered by Brentjes—the Tartaria tablets—must be firmly grasped. Finally, a few words on that much neglected problem, the nature of explanation, are needed in reply to Bhattacharya and Wilmeth.

European prehistory. For the European prehistorian, the late Vere Gordon Childe is a beneficent father-figure whose influence is universal. It was he who rescued prehistory for us from the realm of speculation prompted by isolated and intriguing facts and sustained by elaborate typology. Using the foundations laid by his influential predecessors Worsaae and Montelius, he replaced the squabbling contradictions of limited nationalist studies with a grand survey, based both on a detailed study of the actual cultural assemblages in each area (cf. especially Childe 1925, 1929, 1957) and on an admirably explicit methodology (cf. Childe 1925, 1939, 1956c). To me and to many others, this is the greatest achievement in his work, and indeed in the whole field of Old World archaeology. His pragmatic operational definition of "a culture" (Childe 1929:vi) remains the universal point of departure today, even within the more flexible con-

ceptual framework of numerical taxonomy. Add to this his brilliant analysis of the origins of civilisation in Sumer (Childe 1950, 1952), which is the basis for the further advances of Wittfogel (1959) and Adams (1966), and the contribution is impressive indeed.

The magnitude of Childe's success should not inhibit our trying to improve on it or to reform it. Childe was an avowed diffusionist (Childe 1939; cf. Renfrew 1969), and his explanations for cultural change in prehistoric Europe are not adequate today: in some cases, indeed, they have been rebutted by radiocarbon chronologies. To say this does not imply ignorance of, or lack of respect for, his work.

Similar observations hold for the work of Steward himself, although his ideas (Steward 1955) have not yet been as widely applied in Europe as they deserve to be.

In criticising the adequacy of multilinear evolution as a total explanation I did not mean to deny how very valuable Steward's analysis and extension of the cross-cultural approach, especially his definition of cultural ecology, has been and remains today. From him we have learnt to investigate the relationship between subsistence pattern and social structure: to investigate the interaction of the subsystems within a culture. But the bodily comparison of one culture with another, as Formative, Classic, and so forth, in a sort of typological classification (Steward 1949), seems to me less fruitful, unless combined with a careful analysis of factors within each culture in question.

But this need not obscure that some of Steward's most influential ideas lie at the basis of the whole culture-process approach.

The Tartaria tablets. If one accepted these three objects at their face value, one might indeed, like Brentjes, see them as "the earliest evidence of Oriental trade in Europe (and of real trade at all)" and accept the vision of Sumerian traders proselytising Neolithic Europe. First published by Vlassa (1963), these three small baked clay plaques, bearing incised signs, were found in levels reportedly of the earlier Vinča culture, dated by C¹⁴ (on other sites) to the earlier 4th millennium B.C.—earlier than the Sumerians or their protoliterate predecessors who are supposed to have written them. Milošević (1965) eagerly seized on them in his campaign against the C¹⁴ method itself and enlisted the distinguished Sumerologist Falkenstein to testify to the similarity of the signs with those of protoliterate Sumer. Various scholars, notably Popović (1965), have used them to revive the old theory

of Near Eastern influence on the Vinča culture. For reasons given elsewhere (Renfrew 1969), I believe this quite untenable. Since I was among the first to comment in English on these tablets (Renfrew 1966a, b), it is unreasonable of Brentjes to suggest that I ignore them. Their importance has, I believe, been much exaggerated, and without raising doubts as to their authenticity, there are questions of context and of the validity of the Sumerian comparison to be answered before they can be accepted as proving anything at all.

Explanation in prehistory. Bhattacharya seems entirely just when he argues against a priority in development of trade over the appearance of centralised authority, or vice versa, and presumes "a simultaneous development of both these features." The systems approach is specifically suited to deal with a number of mutually interacting variables in this sort of situation, where none can effectively be isolated. In emphasising the role of trade, I was stressing, or should have stressed, the significance (not the primacy) of one factor causally linked with and inseparable from many others, as Figure 2 is intended to convey. (Smith is working with another in Iran, and it is encouraging that he feels the basic approach valid and useful.) A change in one factor implies a change in the others. In many cases an increase in one implies an increase in the other, and the feedback is positive.

Wilmeth also emphasises the difficulty of "establishing the priority as causal factors of one correlate over another": "the demonstration of this correlation does not provide us with explanations." Here indeed is the crux.

In seeking "explanations" we can no longer hope to ask for a "cause" logically and chronologically prior to the "effects" we are trying to explain. Only occasionally can some Cortes from across the seas be invoked, as a *deus ex machina*, to account for culture change. Nor have universal laws of culture process yet been established by those who seek them. All this may make it difficult to answer "why" a change occurred, but it need not prevent our showing "how." The elucidation of these correlations in itself, that is to say, the adequate description of the behaviour of the subsystems and of their articulation to form the whole, may yet give us the blueprint of the mechanism, the systemic structure of the culture. We can hope for knowledge of the mechanisms which, by regulating the culture, ensure its (relative) permanence and identity, and of those others—trade among them—which generate its growth. Perhaps we shall come to see this as explanation enough.

A number of prehistorians today, including Moberg and those whom he cites, actively imply by the direction of their work a keen awareness of the need to seek new explanations and new kinds of explanation for human achievements and human behaviour in the past. In a few years, I hope, we shall all be rather clearer about the kind of explanation we seek than seems possible today.

Additional note: Binford's helpful and apposite comments, which arrived after the above reply was completed, raise some further points.

The least important is simply one of terminology. I stated clearly a necessary distinction between "civilisation" (with or without cities) and "proto-urbanisation," seen as a prior developmental stage exemplified in the Aegean Early Bronze Age. It may be that this terminology was ill-chosen, since "urbanisation" implies to many people towns/cities and town planning. But there is no confusion here, no failure to distinguish between organisational and distributional phenomena. My point is that the Aegean Early Bronze Age ("proto-urban") constitutes a significant and insufficiently emphasised intermediate stage between Neolithic village farming and Late Bronze Age civilisation in the Aegean.

It was with some hesitation that I wrote of "new needs," since if every

change in the past is to be explained simply by means of a "need" for it, the explanation will be totally sterile, circular, and, as Binford trenchantly puts it, "superficial reductionist." But although we are describing changes on a macro-economic (and not just economic) level, it is necessary to think also on the micro-level. Just what happens when a Neolithic village society is transformed into something more complex, such as the Early Bronze Age proto-urban communities of the Aegean? If whatever innovations occurred were not seen as *preferable* by individual members of the community, they would not have persisted. In my article I was trying to suggest that the potential implied by such "secondary" or "derived needs" (Malinowski 1960:120-21) as prestige or competition was actualised, for example, by the invention of metal tools and weapons. You cannot "need"—at least not without uncommon prescience—tools or weapons which have not yet been invented. As soon as these were seen to be available, demand was created, probably in a sort of positive-feedback process. To put the phenomenon in economic terms, man generally shows both income elasticity of demand and a positive demand-response to new commodities. Yet these new tools and weapons became necessities, not merely luxuries. Hence the "new needs": the

argument is reductionist only insofar as it admits a widespread innate desire for prestige among humans.

In discussing the origins of civilisation, other workers (Childe [1950, 1956b], Wittfogel [1959]), have stressed subsistence factors such as irrigation, and Steward and Adams in particular have emphasised social organisation: here trade has been underlined. Steward, Bhattacharya, Wilmeth, and Binford are right to point out that trade does not have to be regarded as the prime causal factor—but neither, on the other hand, does social organisation or any of the others. These are all components of deviation-amplifying mutual causal systems (Fig. 2) and to isolate one of them is not correct (although to stress trade as a significant one remains appropriate). Unless we keep this in mind, we shall be brought back to the chicken-egg argument pattern: "Which came first, the innovation or the need?" We have only recently begun to use the systems model in this way, and it suggests a different pattern of explanation from those hitherto used. But I think that we are all agreed, at least, that our concept of "explanation" is still far from a final one. I am very grateful to the commentators for their corrections and criticisms, which have helped to clarify the matter considerably.

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